SUPPLEMENT.

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FORMING A COMPLETE RECORD OF THE PROCEEDINGS OF ALL PUBLIC COMPANIES.

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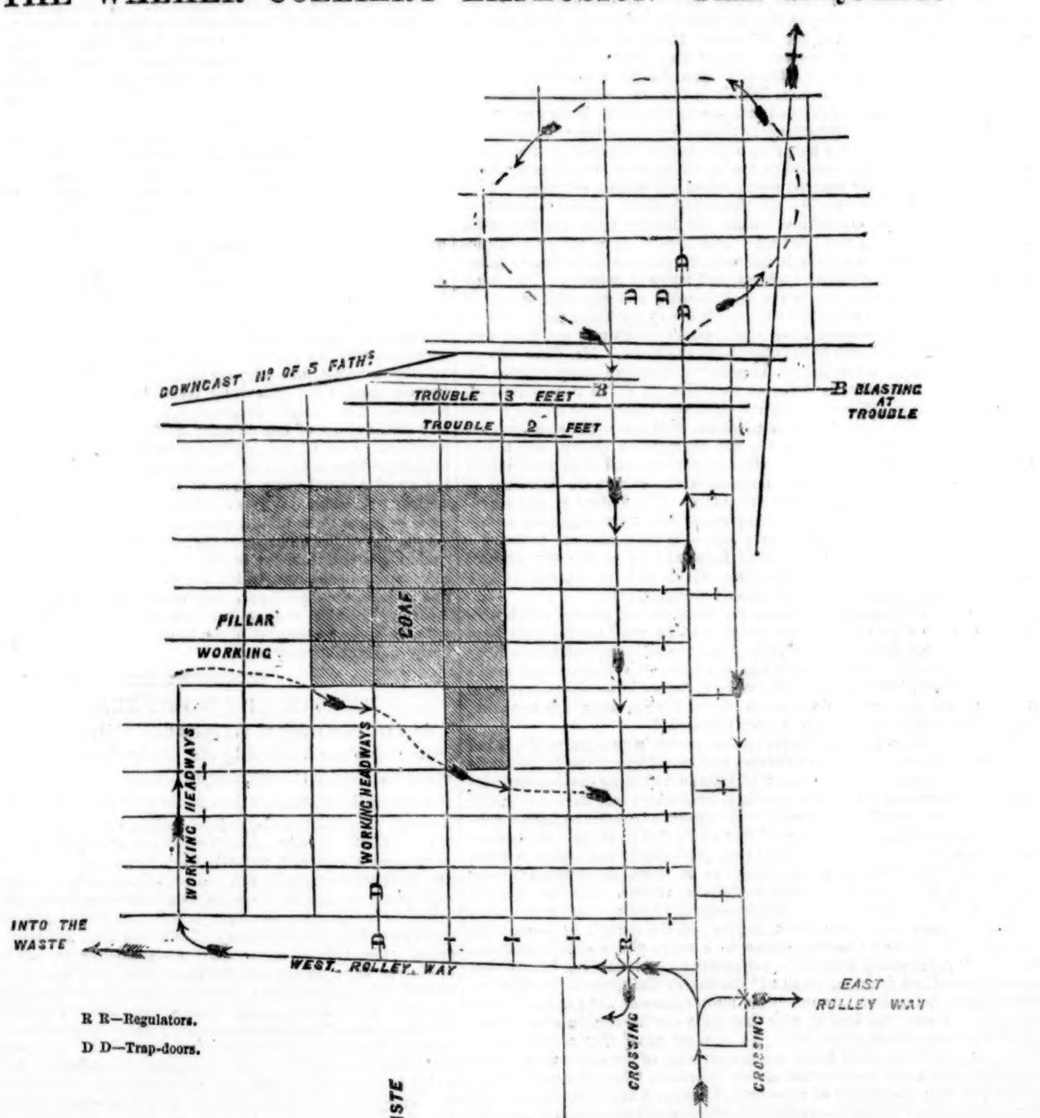
THE WALKER COLLIERY EXPLOSION—THE INQUEST.



From shaft.

We have been favoured by one of the most reliable authorities in the district with a long series of remarks upon the evidence taken at the inquest upon the explosion at the Walker Colliery, and a review of the incidents connected with the calamity, which we gladly publish, in the hope that it may tend to perfect the knowledge necessary for preventing the recurrence of similar casualties.

At the termination of the late Burradon explosion the North of England Institute of Mining Engineers judged it necessary and advantageous to publish a statement of the facts connected with it, as well as the leading evidence taken at the inquest; and, although the discussion thereon was peremptorily quashed, yet it was the means of making known the leading features to distant parties, and doubtless afforded a warning to others to prevent like accidents. So in like manner may the analysis of the Walker explosion be expected to impress the mining community with reflections very advantageous, and well calculated to warn them against similar oversights, as the accident involved the loss of 16 lives, the sufferers leaving eight widows and many children to bewail their unhappy fate.

The preliminary inquest was opened by Mr. S. Reed, on Nov. 25, and upon the jury being empanelled the coroner favoured them with a speech highly laudatory of the ventilation and scientific management of the colliery. It must strike the unprejudiced reader that this was very strange language to fall from a judge upon the opening of an inquest in a case which ought to have demanded the most disinterested and impartial investigation, instead of its being assumed that the subject to be enquired into was of very questionable importance—his remarks bespeaking the most entire persuasion in his mind that no blame whatever could attach to the management of such a model colliery as Walker, conducted, as it was expressed, by the most consummate skill. The inquest was adjourned until Nov. 28, and the Court then presented a remarkable spectacle. Around the coroner's chair were assembled six or seven colliery viewers, including Messrs. Thomas E. Forster, G. B. Forster, W. Armstrong, and others, convened by Mr. Jobling, the viewer of the colliery; whilst Mr. Dunn, the Government Inspector, was unattended by any legal gentleman, and not a single viewer or collier came forward to aid or assist him at the examination. The colliery plan was, it is true, upon the table, but the colliery witnesses exhibited great ignorance of its contents. The audience was of a mixed character, and not more than two or three of the workmen of the colliery were present; none had been summoned, and they were under the impression that their presence would not be agreeable to their employers.

The first witness called was Anthony Barnes, the master wasteman, who, although occupying that position, declared that he had never seen any gas in the pit till that morning, but admitted that there were goaves in that part of the pit. "*It was a mystery to him where the gas came from,*" yet the goaves in question, delineated upon the plan, showed an extent of more than 10,000 square yards of excavations, the coal being $5\frac{1}{2}$ ft. thick, being completely exhausted and unventilated. He also knew that the two innermost bords, connecting the goaf with the trouble where they were blasting, were foul and unventilated, and very high fallen. He admits, also, that to fire a shot the top of the lamp must be unscrewed. The shots were directed downwards, therefore the discharge of the shot and fiery wadding would necessarily point towards the foot of the innermost of these two bords. He had gone through the waste a little before the explosion, and everything was right—all worked with Davy lamps. He was at the trouble (5 fms. downcast) where the men were blasting the stone to enlarge the air-course, and saw one shot fired; it was $1\frac{3}{4}$ hour after that the explosion occurred, yet he says "I do not think they would have time to fire another shot till the explosion occurred." He then fired the furnace hard, and stopped until he was afraid of losing his life from the after-damp; and as this after-damp (mixed with the general return) had to traverse the waste for many hundred yards to that, it must have been very considerable to threaten their lives. He, in fact, says it was coming very strong. He then came to bank at that pit. With regard to the damping of the furnace, the fact is that it was damped 14 hours before the explosion —from four o'clock on Friday till six on Saturday morning. He says the size of the return was 6 ft. by 4 ft. (?). He would venture his life that the explosion did not come from the trouble. This trouble was not supposed of itself to produce such a quantity of gas, but in close connection with it were the foul goafs, and in the process of blasting the fire from the shot would naturally fly towards the bord in connection with the goaf.

John Shield, the back overman, had been through the workings the day the pit fired, and met with very trifling indications of gas. He saw no gas on the low side of the troubles; if there had been gas there it would have passed along the return and up the dyke where Haswell was blasting down stone. He saw some gas two walls north below the trouble. He never saw any gas in the goaf, could not suspect where the gas came from, nor could he tell where the seat of the explosion was, though everybody can now tell where the seat of the explosion was, and also that the goaves were necessarily foul. He says it may have been in the jud, and that they had as much air as usual, which could not be; there must have been more air when both the furnaces were regularly working. He supposes the furnaces were slackened, but there was still sufficient air to work the jud with safety. They had eight hewers in that morning, though even according to Mr. Armstrong it was improper to damp the furnaces when you have the pit at work.

Edward Robinson, the overman, was down the pit at the time it fired, and had examined it that morning. He could not form any idea where the explosion took place; he got a light at the shaft, and endeavoured to get to where the men were lost; he only got about six walls before he had to return; he found the stoppings displaced in the north side of the pit. He then returned, and met Mr. Cole, the resident viewer. He does not think the explosion took place where the men were firing the shot; all the lamps found at the west were all right; he does not think it took place with them. He does not think the gas came from the goaf; he never saw any gas there. This seems ridiculous, considering that it was not at all ventilated. He says there was as much air as usual, though how could that be with the furnace out; and he says that they were remarking just before the explosion what a capital air there was. They found some damaged lamps, and one of them was not locked; it was found in the west jud, just where the deputy's bag was found. In reply to Mr. Dunn's question, "If it had fired from that lamp would it prove the goaf was foul?" he said that he never saw any gas there. With regard to the damaged lamp found in the west bord, and which was a deputy's lamp, and not locked, the explosion itself would readily account for its being damaged. There must have been a great accumulation of gas to produce such an explosion. Robinson thought that the blast came from the north-west, where the goaves are, and of the correctness of this there can be no doubt.

William Mason, lamp-keeper, said all the lamps were in good repair.

Mr. Cole, the resident viewer, read from a book a statement of the amount of ventilation said to exist several weeks ago, which air was said to amount to 61,000 cubic feet per minute, and to be subdivided into seven different columns. Now, Mr. Cole's statement requires to be analysed. The measurements were not his own, but his son's, made some weeks before of this quantity; and, making allowance of a half reduction, there would not be exceeding 4000 cubic feet per minute passing along the headways to the juds, especially under the leakage of the doors below, one boy keeping both. The escape to the crossing would prejudice the effect upon the gas in the goaf. The damping of the furnace and the operations at the Jane upcast pit would cause the air to be diminished, he presumed, one-third; but whilst Mr. Cole only allowed a diminution of one-third, the furnace manager states it one-half, judging from his anemometer. There had been hundreds of shots fired. At the time the accident occurred the blasting was going on below the trouble. Hundreds of shots had been fired at the trouble, but the goaf was constantly in course of enlargement, or it might have happened sooner. He had examined the place where the men where firing the shots since the accident. He could not say whether the explosion took place there or in the workings. There is no doubt it either took place at the trouble or where they were working the pillars in the broken. Judging from the effects, there has been a large amount of fire in some parts; the props are much burnt near the bords. From all appearances he believes the explosion was a very partial one, which, however, could not have been the case, as it involved the firing of a goaf consisting of 10,000 square yards of excavation, besides two foul bords next the trouble. The current of air was restored immediately after the explosion, says Mr. Cole, though witnesses previously examined swear that the stoppings and crossings were destroyed, and that the after-damp was deadly even at the upcast pit. The stable air does not go into the workings, nor does the first east-way air.

The next witness examined was Mr. Thos. E. Forster, who stated that he examined the colliery, particularly at the trouble where the men were blasting. From the appearance of the timber at that point, he was satisfied the explosion did not occur there—the lamps were found there with the caps on, and all secured. He examined the whole bords and the broken, where the greatest amount of damage had been done. It is his opinion that the explosion occurred from either a defective lamp, or from a lamp having been injured. He says his opinion is that the quantity of gas has not been much, an opinion which cannot be understood when the extent and height of unventilated goaf in the position shown in the plan is considered. It should also be remarked that there is no evidence of a defective lamp being in use, but just the contrary, and that the lamp might have been damaged by the explosion. He examined the quantity of air before it is divided into splits; it was 50,400 cubic ft. per minute, a quantity quite ample to ventilate the pit. This statement was quite gratuitous, and palpably erroneous. He said that in all goaves there would be gas, but considered that when the furnace was slacked was quite sufficient, yet the result has proved that it was insufficient, inasmuch as the gas in the goaves was not pressed. He did not consider the explosion happened at the dyke, but in the wagon-way. In reply to Mr. Dunn's question— "Would you recommend the same plan to be pursued for the future as formerly?" he said that he would; he would not have the slightest apprehension of any danger in working the pit again in the same way; and upon this it should be remarked, that a similar answer was given in the Burradon case—that he would warrant the colliery being remodelled in the very same fashion.

Mr. William Armstrong perfectly coincided with Mr. Forster. He could not form the slightest conjecture as to where the gas fired. He did not think it occurred in any of the five bords (and if it did not occur there it must have occurred where the firing was going on), and had no doubt the goaf was foul, and the in-bye bords also. He thinks there was a sufficient current of air—the goaf being so small, and the run of the air-current so short, it was quite safe. Now, the facts and circumstances have shown this opinion to be fallacious. He never saw a pit where there was more care to get a better ventilation. If the accident did not occur in the goaf, he says it occurred in the trouble, and if it did occur there, it is just possible that it might communicate up the bords, and so get hold of the goaf; in the one case you have lamps occasionally exposed, whilst in the other they are never exposed. This part of the evidence is clear and convincing; the lamps were necessarily opened to fire the shots; the discharge was upwards towards the gas, and Mr. Armstrong candidly admits that it might happen there. He never saw a pit in his lifetime where the arrangements were so good and so scientific, and the ventilation so admirably conducted as this. Now, the science consisted in the splits and crossings, but that was overdone—a very common error, and the pit became dangerous. He thought there was ample air when the furnaces were slackened, considering all the lamps were locked; when the furnaces are damped, the pit should not be at work. When you have so small a way for such a quantity of air, as in this case, it is impossible to conceive anything safer. But then Mr. Armstrong seems to deny his own principle just enunciated, for he says, with respect to the theory that has been advanced of the gas having exploded

at the trouble, he holds it to be physically impossible that with such velocity gas could lodge there in such quantities as to explode.

Although the evidence of Messrs. Forster and Armstrong differs in many important particulars, Mr. Johnson, the viewer at Haswell, who examined the pit with them, agrees with both—a very convenient question and answer, "I coincide with them both"—but he seems to deny that any explosion should occur at all. He agrees with Mr. Armstrong that is impossible the explosion could have happened at the trouble; but continues, the goaf would be foul, but there was sufficient air to prevent the gas getting into the workings from the goaf.

Henry Holt, the furnaceman, damped the fire at four o'clock on Friday afternoon, and the fire continued damped until the explosion. The damping reduced the air, but there would still be sufficient to render the pit safe. It is but just to state that this opinion was extorted from Holt, who, of course, could not tell whether in the distant workings the air was sufficient to keep all safe. He had an anemometer to tell the quantity of air, and thought it would be diminished one-half by the damping of the furnace. The damping of the furnace weekly had been going on for some time. The last witness examined was Mr. T. W. Jobling, the viewer, who simply stated that the last accident at Walker occurred three years since, through a man opening his lamp, but what reference this had to the present enquiry it is difficult to discover. The jury then retired, and found a verdict that "the explosion was purely accidental."

LECTURE ON MINERAL VEINS.

BY JAMES NAPIER, F.G.S.

[Delivered at the Glasgow Geological Society, Jan. 29, 1863.]

The subject of the present lecture, although one of my own choosing, is nevertheless a difficult one; and I may at once state that I did not select it because I was able to clear away the difficulties that beset it, or because I had anything new of my own to advance, or even any one of the different theories for the origin of mineral veins to advocate, but to notice a few of the prevailing theories, in order to show that very little is yet known about the matter, and to draw the attention of geologists to this important branch of their science more than has hitherto been done. Geologists in general content themselves with the examination and study of rock masses and their fossils, and too often overlook the little cracks and flaws, filled up with mineral matters, that are to be met with everywhere, as if these had no bearing upon the general physical structure of the crust of the earth, except when they are large, or contain metalliferous minerals that may be commercially valuable. I believe that it is in the smaller and more common sort of veins where the alphabet or rudiments of their history are to be learned; and the laws of their formation may yet be found closely allied to other geological phenomena.

Wherever the rocks forming the crust of the earth have been examined, there are found in them an innumerable number of fractures, less or more vertical to the horizon, varying from a fraction of an inch to many feet in width, and filled with mineral matters differing either in structure or composition from the rocks enclosing them. Many of these cracks are filled with undoubted Plutonic minerals, which have evidently been filled simultaneously with the formation of the crack, and are termed trap dykes; while others have been filled by operations going on subsequent to the formation of the crack, and differ materially in character and structure from the trap; these are termed mineral veins. It is the filling of these veins that has given rise to so many theories, and which has given the subject so great an interest both to geologists and practical miners, and to which I mean to turn our attention this evening.

The question—Whence come the minerals filling the fissures of the rock forming veins? has been put long ago, and answered in various ways by two classes of enquirers—the practical miner and the scientific geologist. The former have consistently maintained an aqueous origin, which education and experience only serve to strengthen. The latter hold various opinions, both igneous and aqueous, and sometimes these opinions oscillate from one to another, according to the bent of some favourite theory; both parties overlook many circumstances and conditions that may yet be found necessary to the true explanation of their origin, such as the composition of the whole contents of the veins, compared with the chemical composition of the rocks enclosing them.

Veins vary very much in their composition. In general they consist entirely of earthy minerals—which, indeed, even when the vein is termed metalliferous, the earths constitute the greatest part thereof—the ore, or metallic mineral, being seldom continuous for any considerable distance in a vein, but is scattered and disseminated throughout the whole matrix or vein-stuff in short irregular veins, layers, bunches, granules, crystals, and other forms. Very rarely, except in small veins, does the metallic mineral prevail. To those whose ideas of minerals veins are always associated with metalliferous minerals, these statements may appear startling, and will, at all events, suggest that any theory founded on such partial views must be defective.

The lecturer went into various details of Cornish mines, showing that no general law has yet been found for the production of veins, and that the apparent law in one locality cannot be applied safely in the search for veins of a similar kind in another; and then went on to consider the various theories for the origin of mineral veins.

Phillips says the local occurrence of metallic veins is in a great measure dependent on the relative antiquity of the rock in the district. It is in the hypozoic and paleozoic rocks generally, and in the igneous rocks associated with them, that all the veins in Great Britain are worked. In a few instances, veins of small value, containing lead and copper, pass through the magnesian limestone, but no true vein is known in the oolite, cretaceous, or tertiary strata. The connection of metallic veins with the older rocks is not an accidental coincidence, but a constantly recurring phenomenon. The same general reference is observed in trap dykes, showing a certain amount of analogy. This analogy between trap dukes and mineral veins is advocated with some ingenuity, in order to prove the igneous origin of veins; and in order to get over the difficulty of trap dykes and veins in the same rock and locality being so distinctive in their character, both chemically and physically, he calls for an exercise of fancy, an element much less likely to be sceptical to hypothesis than philosophy is.

Will it be thought too great a stretch of fancy to attribute this change of the igneous materials erupted in the same tract of country to movements in the internal nucleus of the globe, not isochronous with the rotatory velocity of the solid superficial crust? By such an operation, melted masses of different natures might at successive times lie under the same surface area. Such a thing may be fancied, but I am afraid few will be found to believe it who love truth deduced from facts.

Dr. Collyer speaks more positively, and gives what is probably a fairer view of the true Plutonist. Geological science demonstrates that there was a period of the earth's existence when all substances were arranged in parallel layers or strata. When the cooling process had sufficiently advanced, the hard crust was broken up—mountains were formed, and all the irregularities of violent disruption took place. Coeval with this shrinking, fissures, or huge cracks, were formed; from this source alone have metals been brought to the surface. Simultaneous with the cracking of the surface, igneous liquid quartz and metalliferous materials were forced into the fissures, through which percolated in a dense gaseous form the precious metals, and permeated the interstices and walls of the vein.

M. Agassiz, speaking of the beds of copper at Lake Superior, says—"It must have been of pre-historic origin, that the copper has been thrown up in a melted state, as if it were boiled up. In places where great quantities have come up, and the rock very compact, it has remained unaltered by other influences; but where it was thrown up in less quantity, and the rock not so compact, it has been oxidised and combined with other compounds, as carbonic acid and sulphur. Nearest the metallic beds the copper is found in the state of oxide; then, as we proceed further, it is found as carbonate and sulphuret; and, coming to a greater distance, it is all sulphurets."

Had the Professor favoured some of the other theories, his chemical reasoning might have been reversed with equal propriety. It requires a higher heat to melt copper than ordinary trap-rock, and in the fused state it is very insinuating. Nearly 100 tons of fused copper, in one mass, as at Lake Superior, could not fail to leave evidences of its fused state on the enclosing rock, both by vitrification and penetration, which would have been strong circumstances in corroboration of the Professor's theory. Trap-rocks leave evidence of their igneous origin upon the enclosing rocks. I am not aware of any instance where mineral veins have vitrified the rock, although the heat that would be required to hold their contents in fusion is such that in many instances would melt to a considerable depth the side walls. Mr. E. Rogers lately described a vein of lead about a foot wide in South Wales, that passes up through a seam of bituminous coal, and the coal immediately in contact with the vein bears no mark of igneous action.

Mr. Jukes says, "Where are the minerals, especially the metallic ores, derived from? It must be either directly from original repositories below, or indirectly by segregation from the adjacent rock. If the mineral contents of veins have not been deposited from aqueous solutions either filling the veins, or trickling down their sides, the only other alternative appears to be to support them as the result of sublimation."

The theory of the common miner is that minerals grow. A mining engineer expresses himself thus:—"Every chunk or mass of ore has a root, or is crystallised in cubes on one side, and is amorphous on the other; the root is analogous to fringe and various other excrescences seen in vegetation. Veins of lead ore are formed gradually; their lamellated structure and beautiful angular crystalline surface reminds us of a development in vegetation. In the caves discovered in various parts of the lead regions the ores are frequently found attached to the upper surface of the roof-rock of the cave in string-like veins festooned along the roof of the chamber, analogous of the creeping ivy climbing the walls of some deserted mansion."

A very old mining captain, and one who uses the pen of a ready writer on all matters connected with mining, says, in the *Mining Journal*—"Nearly all the fast crystallising rocks form beds or layers, and carry up mountains, and are too often termed granite; but we see also the lodes keep pace with these mountains—they are found at the very summit. It matters not what the new layers are, the lodes progress precisely as the veins do in the flesh, from the growth of a child until it becomes a man, and, like them, are only covered with a skin of alluvial soil."

In the "Mining Almanac," a guide to miners on matters connected with mines, it is said:—"Every mining district has its conducting metalliferous channels, cross-courses, and feeding pores; and the whole accumulated evidence obtained in all parts of the world clearly proves the fact that the contents of the veins and lodes depend on the character of the rocks they traverse."

These are a few specimens of the positive manner practical miners write upon mineral veins; let us take their facts, and leave their fancies, which may, after all, be as consistent as are many of the highly-educated geologists, when they leave facts to build up hypotheses.

One class of the aqueous theory starts with open fissures or chasms in the rocks, and fills them by segregation, the mineral coming out from the side walls inwards, so that the centre portion of the vein, no matter how wide, must of necessity be the last part filled—that is, if the mineral of the vein oozed out from the side walls, thickening until the two surfaces closed. If this were correct, then the least soluble mineral would occupy the centre. Now, it is difficult to conceive the solution of the less soluble passing through the most soluble in this way, without affecting it. It is not, however, an established fact—or rather, there is no truth at all in the minerals of the veins arranging themselves according to their soluble qualities. It is not even a fact that veins contain minerals that are more soluble than what are contained in the enclosing rock; but in many cases the opposite. Another theory supposes that the cracks, or chasms, have been filled from water entering into them from above, holding minerals in solution which have crystallised within the fissures upon the side walls, until the whole chasm was filled up. This idea is supported by many interesting facts, such as finding in the vein, mixed with the ordinary minerals, masses of rock, rounded boulders, and bones of animals. The latter have been found in the clay in the lead-bearing crevices, with masses of ore 20 ft. above the level of the fossils. It is supposed that these animals have fallen into the fissures when open, which afterwards got filled with clay, and the ore is found in the clay above the bones. Mr. Chas. Moore, at the last meeting of the British Association, describes many veins in the carboniferous limestones in different localities, in which were numerous organic remains of different geological ages, and argues that all our mineral veins, from the oldest to the more recent, were due to the same general law. He discards both the igneous and segregation theory, and thinks that the fissures were open during different periods, and were traversed by the ancient seas, from which the minerals of the veins were deposited or crystallised by electrical and other influences. The finding of organic remains in veins is certainly a most important fact, that ought not to rest without leading to further research; but it is too premature to found a theory upon such a limited number of facts, and to ascribe the greatest part of the work as being done by a force which he has not given a single proof for its existence under the supposed circumstances.

Lyell says—"It may be remarked that those parts of fissures which were not choked up with the ruins of fractured rocks must always have been filled with water, and almost every vein has probably been the channel by which hot springs, so common in the countries of volcanoes and earthquakes, have made their way to the surface, for we know that the rents in which ores abound extend downwards to vast depths, where the temperature of the interior of the earth is more elevated."

Mr. Sorby, in an elaborate paper on the cavities in quartz crystals, found that quartz formed in lavas, or in other undoubted igneous rocks, had their cavities filled with scoriæ. Quartz of granite had their cavities partially filled with water, and quartz crystallised from water had the entire cavity filled with that liquid. He considers the quartz of granite to have consolidated under a pressure of from 18,000 to 78,000 ft. of rock, at a calculated heat of 680°. The moisture at such great depths and pressures could not escape, and was, therefore, held, as it were, in solution in the rock; hence it got into the crystal cavities as vapour. But how the vapour of water came to be at such depths—about 13 miles—into such a temperature, is not stated. Mr. Alex. Bryson, of Edinburgh, in prosecuting the same kind of experiments, comes to a more philosophic conclusion. He says, "After many hundred experiments on such cavities, I found that when exposed to a temperature of 94° the bubble disappeared, the fluid entirely filling the cavity. At the mean temperature of 84° the bubble reappeared. I was thus lead to infer that these cavities could not have been filled at a temperature above 94°. As another proof that these cavities could not have been filled when the temperature of the surrounding rock was higher than the temperature above indicated, I beg to call attention to the fact that the bubble of air occupied always a much smaller portion of the cavity than the fluid—a condition which could not obtain if, as other writers on this subject hold, the fluids were enclosed under intense heat and pressure."

Mr. Sorby found that the quartz in mineral veins had their cavities filled entirely with water, indicating an aqueous origin, and says:—"If the pressure was so great that the water could not escape as vapour, it passed as a highly heated liquid, holding different materials in solution up the fissures in the superincumbent rock, and deposited various crystalline substances to form mineral veins." Mr. Evan Hopkins, an eminent mining engineer, will not admit the existence of open cracks or fissures ultimately filled, but asserts, without troubling the reader with proofs, that all mineral veins are formed by the magnetic currents of the earth, the minerals being accumulated in the veins by magnetism, the rocks on each side pliably yield, and give them place; and, from the fact that all metals are soluble in some acid, hence the various metals have been held in solution in the rock; and thus, he says, "these chemical actions, governed by the subterranean polar currents, continue to fill every fissure or vacuity with crystals, the growth of which swells open the cracks, and thus causes new fractures and dislocations, according to the variable nature of the containing rocks and the amount of resistance. This gradual opening of the veins with the growth of the crystals from the sides accounts for the isolated masses of the bounding rocks found in veins, which could not possibly occur had they been open fractures. Indeed," he says, "the hypotheses, supposing mineral veins to have been filled by solution from above, or that of the injection of igneous matter into an open fissure from below, are so crude and irreconcileable with the nature of their contents that they do not deserve our attention. All veins have been formed and filled on the same principle of polar action as above described.

I come now to consider another class of theories, the facts of which no doubt suggested to Mr. Hopkins his magnetic theory. It was shown by Becquerel and others that certain minerals crystallise from their solutions under the influence of weak currents of electricity. This drew the attention of many who were interested in the enquiry as to the origin of mineral veins; and the further discovery that metals in solution are reduced to the metallic state by an electric current led to further investigation; and the experiments of Mr. Were Fox and Mr. Robert Hunt are very interesting. It was found that by connecting one end of a copper wire with a metalliferous vein, and attaching the other end of the wire to another portion of the same vein, there was no electric currents passed; but if one end of the wire was connected with one vein, and the other end connected with another vein, or cross-course, there was a current of electricity passed between them, in some cases sufficiently strong to decompose certain salts in solution, and in one instance a small electrotype was made by several weeks' action of the current, showing that the one vein was positive to the other; hence it was concluded that this condition of the two veins would effect the concentration and deposition of metals in veins.

Suppose, for illustration, that there are two parallel veins, the one composed entirely of zinc and the other entirely of copper. The mere position of these veins would not produce a current of electricity, and might remain there for ages without the one influencing the other, until they are connected by a conductor, when electricity would then flow from the zinc to the copper, not through the solid rock, which is a non-conductor, but through the moisture in the rock, and it can only affect matters held in solution in that moisture, and only in that portion of the rock that supervenes between the two metals; and the zinc is dissolved and the copper protected. Mineral veins have no such relation to each other. All that these experiments proved was that one vein was undergoing a little more oxidation than the other.

Having briefly stated some of the many theories for the origin of mineral veins, to show that none of them are applicable to all circumstances, I may here refer to a few circumstances in reference to some of the views noticed. The theory that the contents of mineral veins have flowed up into the fissure in a state of fusion from great depth is, I think, surrounded with the greatest difficulties. The disposition of the minerals, in mostly all cases, favours slow formation. If injected as fused matter, such as trap, by pressure, it must have been sudden, and, in this case, the enclosing rocks [illegible] have been affected, and the physical structure of the contents of the ve[illegible] must have been completely metamorphosed, and in a way necessitating subsequent solution and rearrangement. Such masses of pure metallic copper as formed at Lake Superior may flow into a crevice in a melted state; but then whence comes the fused metal *so pure?* A metal whose affinity for other metals and mineralising substances in a fused state is so great that it is the most difficult metallurgical operation to separate them; and here we have pure copper flowing up, separated and purified from all other ingredients; indeed, it almost leads one to adopt something like Professor Phillips' idea, that the different metals are held as in compartments under the crust; but where the metallic copper is found in veins it is generally in a bunch or isolated mass, resting upon and mixed up with minerals having only one-third of the specific gravity of the copper. Another fact may be referred to. Melted copper, when examined by the microscope, has invariably a cellular structure. Native has not this structure, but is either crystalline or massive, and when subject to fusion assumes the cellular structure. As to sublimation, that mineral bodies are held in a gaseous condition in mines, is a fact beyond dispute. Dr. R. A. Smith has found recently that glass tubes, filled with the common air of mines, and hermetically sealed, deposited minute crystals upon the surface of the glass in a few days, which he found to be sulphate of potash, probably originating in the gunpowder used in blasting; and many minerals when held in solution are carried off in the vapour of their solvent, and deposited upon solid bodies. I pointed out some years ago, in a paper to the Philosophical Society, that minerals are often found crystallised upon one face of a projecting crystal or stone, and not on the opposite side; so that sublimation is a means for carrying mineral matter from one locality to another in fissures or mines, but only a very small means as applied to veins.

The experiments of Sorby and Bryson favour much the theory that the quartz, and consequently the other minerals of the vein, have had an aqueous origin; and I hope these observations will be greatly extended, as no doubt much is to be done by the microscope, as well as chemistry and other kindred sciences. But although these experiments point to an aqueous origin, we cannot say whether, as Werner supposed, the water containing the minerals in solution entered from the surface into the chasm, or, as others suppose, by segregation, the water percolating the rocks, dissolving out the minerals, and carrying them into the fissures; or as Lyell, by the fissures being filled with water from hot springs containing minerals in solution. Mr. Moore's observation, if found to be general, that veins contain fossils either of one or more geological periods, is of the utmost importance. I am of opinion that most of these causes, and probably more than have been referred to, have been at work in the constructing and filling up of mineral veins, some of them in certain localities more than in others; but, before any general theory can be formed of their origin, there must be much more minute observation made upon the *whole contents* of a vein, physically and chemically, also the nature and composition of the enclosing rocks, from the surface down to the depth of the mine, and the composition of the rock within the water-shed leading to the vein. When this is done, then geologists may venture upon a theory for the filling of veins, and be able to show how one part of a vein is rich in one kind of mineral, and another part of the same vein in another sort of mineral entirely different in every sense.

ECONOMY OF CORNISH PUMING-ENGINES.

At a recent meeting of the Institution of Mechanical Engineers a very interesting paper was read by Mr. Charles Greaves, "On the relations of power and effect in Cornish pumping-engines over long periods of working." In working the Cornish engines at the East London Waterworks the steam is raised in cylindrical single-flued boilers, with internal fires, to a pressure of 30 to 35 lbs. per square inch above the atmosphere; the boilers are of ample dimensions, and not less than three are at work for each engine; they have large steam chests, and are all covered up with great care. The engines are worked at all speeds, from four to ten strokes per minute. The cylinders are all cased in steam jackets, and these again are enclosed in an outer case, filled to a thickness of not less than 12 inches with very fine ashes. The cylinder-covers have no steam-jackets, but are well covered in various ways, as are also the steam-pipes and upper nozzles, or valve-boxes. The steam-valves are in all cases double-beat gun metal valves, and in as good order as close care and attention can maintain them. The condensed water from the steam-case returns by gravitation to one of the working boilers, the cylinders being purposely at such a level relatively to the boilers as to allow of this continued circulation by mere gravitation, which is not interfered with by the working of the engine, continuing to act during the intervals of work, or as long as steam remains in the boiler. The speed of the engine is regulated by an adjustable cataract: th[illegible] haust valve first, and then the steam-valve, are thrown open by tr[illegible] weights as soon as the catches are detached by the cataract. The va[illegible] are closed by tappets on a plug rod—first the steam-valve, then the exhaust-valve—the former at from one-third to one-fifth of the stroke, the latter at the end. In engines worked on this principle, as also in all reciprocating engines without cranks, there is nothing to limit the strokes of the engine to any exact length. It is necessary, therefore, that bumpers or catch-pieces be provided, to restain the engine at both ends from an undue length of stroke, and thick plates of India rubber under hard wood blocks are now used for this purpose, in place of the spring beams that were formerly employed.

An engine thus arranged, working alone, lifting water from one fixed level to another, would work continuously with one length of stroke and one speed at whatever it might be set; but in waterworks with direct delivery—that is, not pumping into a stand-pipe of constant height of column, but where the levels of the reservoirs vary continually, and the velocity of delivery into the main pipes is subject to continual fluctuation—it is found that a variation to the extent of some inches in the length of stroke results throughout the day, and the engines lengthen and shorten their strokes in obedience to the variable resisting pressure of the column of water. Each stroke of these engines is an operation complete in itself, including within itself all the changes from rest to rest, and there is no momentum carried on, and no arrear of force subsequently supplied. The indoor stroke is performed at the mean velocity of from 500 to 600 ft. per minute. This speed in pumping is almost peculiar to waterworks' engines, for in mining engines the same length of stroke generally requires double, or more than double, the time. Much depends on this velocity. The chamber of the pump having to be filled during the indoor stroke, the dimensions of the suction-valves must be such that the least loss of power may be suffered in drawing the water in, and the adoption of double suction-valves has been very beneficial in economising power. Moreover, it is absolutely necessary for the good working of the engine that the suction-valves should be shut quite as soon as the engine concludes the indoor stroke—the lift and loading of the valves are matters requiring considerable attention.

In ordinary registers of steam-engine performance it is thought sufficient to give a comparison of the amount of work done, in weight of water lifted to the known height, with the weight of fuel necessary to carry the engine through that amount of work. There is, however, a defect in the limited information thus given, since it includes in one statement the whole efficiency of the pump, the engine, and the boiler. If an additional register of the quantity of water ordinarily used as steam can be added, it becomes

possible to discriminate between the efficiency of the boiler and of the engine, and to investigate the economy of the engine itself, without the complication and variations arising from different constructions of boiler, size of fire, quality of coal, and ability of stoker. The same measure of the water boiled off is no less a most sure comparative test of the good qualities of the boilers and the fuel. The object of the author, however, is to show the quantity of water used as steam in performing the work, with the proportion which the work actually done bears to the theoretical power of the steam, as deduced in both cases from the final degree of rarefaction of the steam at the conclusion of the stroke. The true measurement of efficiency in a steam-engine is the quantity of feed-water used, as has been well shown by De Pambour; and Mr. Greaves, having endeavoured to carry out the same plan of measurement, can testify most thoroughly to the very exact knowledge of the condition of an engine that can be obtained by making the water evaporated per stroke one of the elements of continual registry. There are four engines at work at the East London Waterworks, and Mr. Greaves has furnished elaborate tables of the results which he has obtained, not from experimental or exceptional trials, but from actual experience, extending over long periods of time. The subjoined abstract will, to some extent, explain the contents of the tables:—

Cylinder in inches	72	80	90	100
Area of piston, square feet	28·27	34·91	44·18	54·54
Length of stroke, feet	9·62	9·75	10·58	11·00
Capacity of stroke, cubic feet	271·95	340·37	467·42	599·94
Total load on piston, lbs. per square inch	15·00	14·38	15·58	16·58
Point of cut-off	1-4th	1-3d	1-4th	1-4th
Time in use, years	3	2	4	3½
Total strokes in that time	5,466,090	2,981,470	8,632,816	9,296,214
Total gallons feed water evap.	3,359,774	2,269,850	8,605,340	13,505,605
Average per stroke, gallons	0·615	0·761	0·997	1·453
Average per stroke, lbs.	6·15	7·61	9·97	14·53
Average per stroke, cubic feet	0·0987	0·1222	0·1600	0·2332
Actual final expan. of steam	2755	2781	2921	2537
Theoretical final expansion	3985	3638	3846	3648
Difference per cent.	31	24	25	29

The quantity of feed-water evaporated per stroke varies very little from the average in any of the half-yearly periods recorded, the difference seldom exceeding a few 1-100ths of a gallon, and the greatest difference being ·042 gallons, or less than half a pound, and that is in the 100-in. cylinder engine. Mr. Greaves concludes his paper by observing that the standard quantity of feed-water required to produce a stroke of known effect having been obtained from the average of so long a period of working, it must be remembered that there are probably several causes by which the consumption of feed-water per stroke, as stated in the preceding table, may have been accidentally increased. The inaccuracy that might arise from blowing out the boilers while in work has been entirely avoided; but there is a constant liability to loss from possible unknown leaks from safety-valves and gauge-cocks. The chance of spare boilers put on short of water, or put off with excess, may be balanced by a contrary proceeding. Extra steam is used in starting, which must tell up with the engine working only 12 hours out of the 24. The causes by which to explain the difference between the actual power obtained from the steam and the theoretical full power are—the friction of the engine; the possible leakage of the piston, of the steam-valve while the piston is in partial vacuum during the out-door-stroke, and of the equilibrium valve while the steam is on the piston during the indoor stroke; and the imperfection of vacuum in the condenser, since it is not to be supposed that an air-pump is all the year round in a condition to work at all hours within 1·66 inches of mercury of the atmosphere. Then the cooling of the piston-rod during the exposure of every stroke, the condensation of steam on the cylinder cover, which has no steam-jacket, and the condensation, if any, of steam on the sides of the cylinder itself, which would be evaporated again and pass away through the exhaust-valve into the condenser, are evident sources of loss continually operating. These it is the duty of the engineer to use every means of diminishing, in pursuit of that theoretical economy which would result in still further reducing the difference that yet remains between the power expended and the useful effect produced; and an important step towards the attainment of this object will be to ascertain an experimental value of the loss arising from each source.

In an interesting discussion, which followed the reading of the paper, Mr. Greaves stated that he had found that a cut-off at one-fourth stroke was a very convenient degree of expansion for regular working in such engines. The engines worked at the rate of 1s. per horse power per day of 24 hours, including all expenses and every kind of repairs, but not interest on capital. Mr. D. Adamson remarked that the arrangement of the Cornish engines described in the paper, appeared to involve a very large outlay in the first cost of the engine, in proportion to the amount of power obtained since the mean pressure of steam throughout the stroke was stated to be only 2 or 3 lbs. per square inch above the atmosphere. He thought the application of large cylinders, with low pressures of steam, was not an economical or advantageous mode of working; and that to get the greatest economy it was necessary to develope the largest amount of force from the steam side of the piston, instead of obtaining more than three-quarters of the entire power from the exhaust side of the piston. Moreover, the Cornish engine being single-acting, the whole power required for performing the work had to be put into the engine in one stroke, instead of being equally divided between the two strokes; and with so low a pressure of steam as was generally used, and an early cut-off, a very large and expensive construction of engine had to be employed for performing a comparatively small amount of work. With pressures of 140 to 160 lbs., now employed successfully in locomotives, there seemed no reason why the required power should not be obtained in stationary engines by the use of much smaller cylinders, working double-acting, and steam of 100 or 120 lbs. pressure, which, with suitable boilers, would be easily practicable, while the engines would run steadier, and would involve a much less extensive accommodation for housing them. At his own works he had had such an engine, of about 42 indicated horse-power, working regularly for 8½ years, with 150 lbs. of steam, and with a consumption of 2⅛ lbs. of coal per indicated horse-power per hour; and the first cost of the engine, with boilers complete, was not more than 20 per cent. of the outlay that had been mentioned for the Cornish engine. He maintained that the single-acting beam-engine, with loaded plunger, was clearly preferable to a single-acting crank-engine; but with a double-acting engine, with crank and fly-wheel, and with a higher degree of expansion, he believed more power would be obtained from a given consumption of fuel than could be got in the Cornish engine. For the purpose of driving machinery the Cornish engine was admitted to be altogether inapplicable, from the great variation in the power throughout the stroke: but even as a pumping-engine he thought its real economy had been over-rated, since the most economical results were said to have been attained with pressures of only 25 or 30 lbs. above the atmosphere at the outside; and if this were the case a still greater degree of economy might be expected to be obtained by the adoption of higher pressures of steam.

CHINESE BLACKSMITHS.—We have been favoured by a correspondent with the following extract from a letter just received from Shanghai:—

The smiths' shop is about 100 ft. long and 30 ft. broard, and we employ about thirty smiths, all Chinamen; in fact, they make very good smiths. I may mention that all their tools are the same as are used at home—English anvils, same style of sledge hammer, bellows, creases, tongs, &c. They are very slow workmen, although they make a very good job of some things. The country smiths have not the same tools as we have, nor do they work in the same style. The bellows which they have have two valves at each end. There are two rods which go through the end of the box, and are attached to a wooden piston inside, which is thickly covered with feathers round the edges. There is a handle attached to these two rods, by which they move it out and in; it is a very ingenious and simple affair, and they can weld iron about 3 in. diameter with these bellows, but it takes a long time before they can get it hot enough. Their fire and hearth is a round earthenware pot about 2 or 3 ft. diameter, and stands about 18 in. high. At work they are mostly sitting at it—that is, at small jobs.

NEW STYLE OF CANAL BOAT.—From America we learn that a canal boat, built entirely on a new plan, is in progress of construction at Ithaca. Instead of the usual frame, planked on the outside and sheathed within, this boat is composed of solid sticks of timber, breaking joints, and lying one upon the other, clamped down together with heavy bolts, and braced with strong stays throughout. Some persons of experience in canal navigation claim that this boat will have greater capacity for her size than other boats, as she will be lighter, and at the same time stronger and more substantial.

PETROLEUM.—Few persons are aware of the importance to which petroleum, or rock oil, is growing, as an article of commerce. In the course of another year it will rank with cotton, corn, provisions, wheat, and tobacco among our staple exports. There appears to be no limit to its production, and there is certainly none to the demand for it at home and abroad. We understand that a line of steamers are to ply next summer between New York and a British port solely for the purpose of conveying it to Europe. They are to be built in compartments, and to carry the oil in bulk. This will be good news to the owners of the petroleum districts.—*New York paper.*

MARYLAND COAL TRADE.—During the past year there were shipped from the coal region in Alleghany county, Md., 100,804 tons of coal. The Chesapeake and Ohio Canal continues open, and boats are daily leaving Cumberland.—*New York Herald*, Jan. 17.

THE MANUFACTURE OF COBALT AND NICKEL.

BY LEWIS THOMPSON, M.R.C.S.

Not many years ago the whole scientific world was thrown into a kind of convulsion by the announcement that Mr. Faraday had discovered "that pure cobalt was altogether destitute of magnetic qualities." Coming from such a source, the assertion met with more respect than, as events proved, it deserved, for in a short time Mr. Faraday announced another discovery, to the effect that he had been mistaken, and that pure cobalt was highly magnetic. Now, there are two useful conclusions to be deduced from this circumstance, and I recommend them strongly to the attention of my chemical readers. In the first place, Mr. Faraday's mistake shows the value of the old Roman axiom, "*In verba magistri non esse jurandum;*" and, in the second place, it proves that pure cobalt is a scarce article, otherwise it would have existed in the laboratory of the Royal Institution. Bearing, then, both of these conclusions in mind, I hope my readers will discover something to interest them in the following observations.

It is usual to find a statement in chemical books to the effect that the only difficulty in making pure cobalt or pure nickel consists in separating the one of these metals from the other. This, however, is very far short of the truth; for the complete separation of arsenic, manganese, and zinc from both or either of the above metals, is really more difficult and tedious than the separation of cobalt and nickel. It would take me greatly beyond the confines of my present purpose to enter fully into this subject, nor is it absolutely necessary, except so far as arsenic is concerned; for although manganese and zinc are much more frequently associated with cobalt and nickel than is generally supposed, yet many ores of the latter metals are to be had, which contain neither zinc nor manganese; I shall, therefore, merely say a few words upon the separation of arsenic, with which almost all the ores of cobalt and nickel are contaminated. Everybody has been told how to effect the separation of arsenic by passing a current of sulphuretted hydrogen in excess through the solution, then heating, filtering, and so forth; but with solutions of cobalt this will not do, for a portion of arsenic always remains, as may be very easily demonstrated by a modification of Marsh's test. Thus, having gone through the prescribed form of separation as above, let a part of the purified solution be submitted to the action of a strong galvanic current passing through two platinum wires or poles, and whilst the current is passing, let the hydrogen alone be collected from its proper wire: this hydrogen, when heated or burnt, will be found to yield arsenic in abundance. I have no hesitation, therefore, in asserting, that for the purpose now in hand, the process of arsenical separation first described by Wöhler, is infinitely the best; with this difference, however, that the ore should be first dissolved in nitric acid, and the clear solution evaporated to dryness, the heat being ultimately raised to a bright red. The residuum thus obtained must then be finely powdered, and mixed with four or five times its weight of dry sulphuret of potassium,* after which the mixture is to be heated to redness, or until it fuses, in which state it should remain for half an hour, when it may be poured out to cool and solidify. The solid matter is next to be broken into small pieces, and boiled in water containing a little caustic potash. This mixture, on being set aside, very soon becomes clear, and the clear liquid is then to be poured away, and fresh water added, as before, so as to repeat this boiling and washing, until a portion of the clear fluid no longer evolves the odour of sulphuretted hydrogen when super-saturated with an acid. The black powder which settles down at the bottom of the fluid is a metallic sulphuret, consisting chiefly of the sulphurets of cobalt and nickel; but, after a sufficient washing, it contains neither arsenic, antimony, selenium, nor phosphorus; so that if an ore has been employed which is free from zinc or manganese, there will be no difficulty in extracting from this black powder a mixture of the oxides of cobalt and nickel, perfectly exempt from every other metallic contamination. Thus, then, we arrive at a pure mixture of the oxides of cobalt and nickel, and the point now is, how are we to separate these two oxides?

A great many processes have been recommended to us for this purpose, not any one of which, according to my experience, will completely answer the desired end. I will, however, here give a short epitome of my experience upon this subject.

1. The process of Phillips is to dissolve the two oxides in solution of ammonia, and then to add a solution of caustic potash, under an idea that the oxide of nickel alone would be precipitated by this means. In reality, however, a large portion of the oxide of cobalt, frequently more than the half, is also precipitated with the nickel, although the clear solution is quite free from nickel, and will, therefore, afford pure oxide of cobalt.

2. Berthier's process is to pass a current of chlorine through an aqueous mixture of the two hydrated oxides, under an idea that by the decomposition of the water, or the transference of oxygen, the protoxide of cobalt would become sesquioxide, and the oxide of nickel become chloride or muriate. If this process were good for anything it could be useful only when the mixture of the oxides consisted of two atoms of cobalt to one atom of nickel; but even then, as I have ascertained by experiment, this process is good for nothing.

3. The process of Laugier is to dissolve the mixed oxalates of cobalt and nickel in an excess of the solution of ammonia, and expose the whole to the air, under an idea that the oxalate of nickel alone would precipitate, and nothing but cobalt remain in solution. This is altogether wrong. The precipitate contains both nickel and cobalt, and so does the clear solution, even after a three months' exposure to the air.

4. The process of H. Rose is a modification of that proposed by Berthier, in which, however, the action of the chlorine is aided by the effect of carbonate of baryta and the use of "a large quantity of water." I do not know how much a large quantity of water is, but I have tried the process within everything like reasonable limits, and it has completely failed.

5. Liebig's process has been given up, even, I believe, by its proposer. It certainly does not answer. It consisted in dissolving the cyanides of the two metals in a solution of the cyanide of potassium, then, after boiling and cooling the solution, adding an excess of diluted sulphuric acid, under an idea that the nickel only would precipitate. I have, however, found both cobalt and nickel in the precipitate, and the same in the solution.

It now remains for me to describe my own process, for which I may naturally be supposed to entertain an undue predilection, consequently my readers will do well to try the process for themselves, and not forget the old Roman axiom previously quoted. To begin on, however, I must stipulate that the mixture of the oxides of cobalt and nickel ought to be free from arsenic, antimony, phosphorus, alumina, silica, magnesia, and organic matter; and although I am about to give definite proportions, yet these need not be very rigidly adhered to, as a little experience will soon demonstrate:—Take 30 grs. of the mixed oxides of cobalt and nickel, and dissolve these in a slight excess of hydrochloric acid. When dissolved, add 100 grs. of pure chloride of calcium, and the same quantity of hydrochlorate of ammonia, after which heat the mixture, so as to expel the superfluous hydrochloric acid. Now pour in 3 ozs. of cold distilled water, and place the whole in a Florence flask, or other convenient vessel. Next add to this 200 grs. of the sesquicarbonate of ammonia, previously dissolved in 2 ozs. of cold distilled water, and after agitating the whole together, heat the mixture gradually up to the boiling point; then set it aside to cool and precipitate. When cold, throw the whole upon a filter, and wash the precipitate with a solution of the sesquicarbonate of ammonia, taking care to add the washings to the clear solution. This solution contains the nickel only, and the cobalt may be separated from the precipitate by either one of two ways:—First, dissolve the precipitate in hydrochloric acid, and after evaporating almost to dryness, add 15 grs. or 20 grs. of powdered chlorate of potash; then heat the whole to about the boiling point of mercury, or to a dull red heat. In this way the cobalt becomes converted into the insoluble sesquioxide, and the alkaline and earthy salts may then be removed by water and filtration. Or, secondly, having, as before, dissolved the precipitate in hydrochloric acid, and evaporated to dryness, re-dissolve in water, and after adding 30 or 40 grs. of precipitated peroxide of mercury, boil the whole together; then collect, wash, and dry the precipitate, and afterwards heat it red-hot, to obtain the oxide of cobalt.

The *rationale* of this process does not admit of a very easy explanation upon chemical principles, for although we find in nature, and particularly within the confines of mineralogy, abundant evidences of the effect of a kind of affinity such as occurs in this case, yet it scarcely comes under the meaning of the term chemical affinity. If to a mixture of the chlorides of cobalt and nickel we add an excess of a solution of the sesquicarbonate of ammonia, the whole remains in solution, even if we boil the fluid. If, however, to a mixture of the same chlorides we add a portion of chloride of calcium before pouring in the solution of sesquicarbonate of ammonia, a precipitation of carbonate of lime ensues, and this carbonate of lime takes down with it the whole of the cobalt, but leaves the nickel in solution. This result is facilitated by the employment of the hydrochlorate of ammonia, and heat, in the way just described. The precipitate, then, is a mixture of carbonate of lime and carbonate of cobalt united, as we find carbonate of lime and carbonate of strontia in arragonite, carbonate of lime and carbonate of magnesia in dolomite, carbonate of lime and carbonate of iron or manganese in pearl spar, and so on. But the kind of affinity by which they have been made to unite together is not very apparent. It is unnecessary for me to explain the principles upon which the lime and cobalt contained in the above precipitate are separated from each other; it will be, perhaps, enough for me to say that, by this means, I have separated 1 gr. of cobalt from 1000 grs. of nickel; and that I have detected cobalt in every specimen of nickel or oxide of nickel sold to me, as quite pure, by the most respectable dealers. It will be apparent, from the proportions given above, that the quantity of lime, with regard to the cobalt, requires to be very large, and this is absolutely necessary to ensure complete success. As a general rule, the proportion of ten of lime to one of cobalt may be assumed, and this, after a little experience, is easily adjusted by guessing at the relative amounts of cobalt and nickel from the colour of the hydrochloric solution. Indeed, the colour forms not only a delicate test for cobalt, but also an infallible indication of its purity. If the cobalt is pure, the solution, when evaporated almost to dryness, takes on a pure blue tint, like indigo, without the least tinge of green. The chloride of vanadium is also blue, but differs from the chloride of cobalt in this respect—upon dilution with water the cobalt becomes either colourless or a pale rose tint, whereas the blue colour of the vanadium remains unchanged. Having thus explained the mode of obtaining pure oxide of cobalt, I will proceed to speak of the metal itself, merely premising that, after an examination of many hundreds of samples of the different articles sold in this country, and in France and Germany, under the name of pure oxide of cobalt, I have never met with one containing less than 5 per cent. of impurity. This impurity varies in its composition, and includes within its range alumina, silica, arsenic, phosphorus, iron, zinc, manganese, lime, and magnesia.

To obtain metallic cobalt, I mixed together two parts of pure oxide of cobalt and one part of pure cream of tartar (bi-tartrate of potash). This mixture was placed in a crucible, lined throughout with charcoal to a depth of 1 in. A lid was luted on, and the whole exposed for six hours to the highest heat of a steel furnace. In this description I speak only of one crucible, but in reality there were three, all of which came out of the furnace safe and sound, and afforded, therefore, three metallic nuggets, of the collective weight of 22 ozs. Upon analysis, this metal was found to contain about 4 per cent. of carbon. It was very hard and brittle, had a specific gravity of 8·43, a colour something like bismuth, and when magnetised retained its magnetism like steel. To remove the carbon, it was re-melted in a crucible lined with pure alumina; and to the metal, broken into fragments, a quantity of flux was added. This flux consisted of two parts of pure oxide of cobalt and one part of pure carbonate of potash, previously fritted together, and then reduced to powder. A lid was luted on the crucible, and the whole submitted, as before, to the highest heat of a steel furnace for eight hours. Of three crucibles, only one came out of the furnace in a perfect state. This, however, furnished me with about 7 ozs. of what I hoped might prove to be pure cobalt. It was of a bright silver colour, with a very trifling shade of yellow; its specific gravity was 8·754; it was much softer than steel, and so decidedly malleable, that under the hands of a workman it spread out into a plate about 12 in. long, 10 in. wide, and not more than ⅛ in. in thickness. It did not tarnish or oxidise by exposure to the air, nor even when kept for days under common water. Its magnetic properties appeared quite equal to those of iron, so far as could be judged by the relative effect of two equal pieces of those metals upon a magnetised needle. Unfortunately, a very careful analysis disclosed the painful fact that this cobalt was not absolutely pure. It contained no carbon, but afforded a minute quantity of boracic acid and alumina, in the proportion of one part of these impurities to 470 of cobalt; at the same time, I have no doubt that these impurities existed in the metal, in the form of boron and aluminium. To those who have had but little experience with metallic alloys, the quantity of impurity here described may seem too small to be worthy of notice; nevertheless, from what I have seen in regard to the effect of arsenic and other metals upon iron, I do not consider that my experiments have fairly elucidated the metallic properties of pure cobalt, though they encourage the hope that this metal may be very useful, both in respect of malleability and power to resist atmospheric influences.

I will now say a few words about nickel, and, first, about the substance sold as nickel in this and other countries. Commercial nickel is a very impure article, and bears no more relation to pure nickel than brass or bell-metal does to copper. The following table will show its average composition, as it is found in the market:—

	English.	English.	German.	German.	French.
Nickel	86·0	84·5	75·7	80·9	77·3
Cobalt	6·5	8·2	2·2	5·2	3·7
Copper	—	0·6	12·5	7·7	10·2
Iron	1·4	1·1	0·4	1·2	1·1
Arsenic	1·3	0·4	2·6	3·8	2·8
Zinc	2·0	0·7	4·1	0·5	1·4
Manganese	0·2	0·8	—	—	0·6
Sulphur	1·7	2·2	2·3	0·2	1·1
Carbon	0·5	0·9	0·2	0·1	0·7
Silica and Alumina	0·4	0·6	—	0·4	0·9

From what I have before said, there is every reason to suppose that our accounts of metallic nickel relate to an alloy of that metal with cobalt, in greater or smaller proportion; that, in fact, absolutely pure nickel has not hitherto been obtained. Pure nickel is, however, much more easily made than pure cobalt, for its affinity for oxygen is much less. Taking advantage of this fact, I made up a quantity of pure oxide of nickel into a paste by means of a little water, and forced this paste through a perforated earthenware plate, so as to form it into a granulated mass; when this mass had been thoroughly dried I introduced it into a porcelain tube, and, after heating it red-hot, I passed a current of pure hydrogen gas over it, and continued this until it had become cold. The grey metallic sponge thus produced was fused with a little borax, in a crucible, lined with pure alumina, and yielded a beautiful white silvery-looking button, of the weight of 620 grains; its specific gravity was 8·575, and it was almost as soft as copper. Its malleability seemed very great indeed, for a piece of it was rolled out nearly to the thinness of tinfoil; it showed, however, a disposition to tarnish after a few days' exposure to the air, and became then of a pale yellow colour—a kind of green-sickness tinge. Its magnetic properties were less decided than those of either cobalt or iron; and, judging by the globular form and other evidences of perfect fusion in the button, I believe that nickel is much more fusible than the two metals just mentioned. When portions of it were melted with copper and zinc, in the quantities usually adopted to form albata, it produced a compound vastly superior in appearance to any of the miserable make-shifts that now disgrace our markets. Indeed, I am quite convinced that it would well repay any respectable person to commence the manufacture of pure nickel; and it would not surprise me if a compound of aluminium and nickel could be formed which, for beauty and appearance, might equal silver, and surpass it in durability and freedom from sulphurous deterioration.

Whilst alluding to the advantages of an improvement in the manufacture of nickel, it may not be amiss for me to notice two points of some importance in the way of improvement. At present the extraction of nickel from the ore is made to depend very much upon the affinity of arsenic for that metal, so as to form with it an arseniuret of easy fusibility, and sufficient specific gravity to separate freely from the melted slag or gangue; and for this purpose large quantities of arsenic are employed by the workmen, not only to the detriment of their own health, but also to the injury of their neighbours. This pernicious practice is quite unnecessary, as I have myself proved by experiments upon a large scale; for example, after carefully roasting 6 cwts. of the common ore of nickel, which is an arsenio-sulphuret, I mixed it with half its weight of chalk, and threw the mixture into a cubilo furnace in full blast; the result was, that the lime of the chalk formed, with the quartz and oxide of iron in the ore, a perfect flux, whilst the oxide of nickel, being easily reduced to the metallic state, fell, in that condition, into the well of the cubilo, from whence it was run out in a melted form, and readily separated from the slag. There was no appreciable loss of nickel in this operation, and the rough metal was found to contain 88 per cent. of pure nickel, the rest being cobalt and iron, with a little sulphur, but no arsenic could be detected in it; moreover, this rough metal might, from the cheapness of the process, have been profitably sold at 3s. per lb., and was decidedly more pure than the ordinary commercial metal.

The other point to which I have alluded is applicable to the wet mode of separating nickel, and depends upon a fact hitherto, I believe, unnoticed by chemists. If we have in solution a mixture of the sulphates of nickel, cobalt, zinc, manganese, iron, and copper, we have only to add to this solution, in a warm state, as much sulphate of ammonia as it will dissolve, and then set it aside to cool. Almost every particle of the nickel and cobalt will separate as a green crystallised powder, and leave the other metals in solution. The explanation is very simple. The sulphates of nickel and cobalt form triple salts or alums with the sulphate of ammonia, and these salts are absolutely insoluble in a cold saturated solution of sulphate of ammonia, particularly when this solution is slightly acidulous. I shall conclude these remarks upon nickel, by stating that this metal appears to possess the property of "welding" like iron. At my request, a workman heated two small bars of nickel, which had been previously powdered over with borax; the bars were heated in a forge, and two hot ends "jumped" together; that is to say, the white-hot ends were forcibly driven one against the other by gentle blows with a hammer, applied to the other ends, the symmetry of the bar being preserved by blows applied laterally. Although the point of junction was afterwards subjected to much twisting, straining, and so forth, with a view to test its cohesive power, yet it showed no signs of weakness, even after much cold hammering.

—*Newton's London Journal of Arts and Sciences.*

* By sulphuret of potassium I mean the compound formed when one part of sulphur and four parts of dry carbonate of potash are fused together at a dull red heat; in fact, the "liver of sulphur" of old writers.

WILLIAM JAMES, THE ORIGINAL PROJECTOR OF THE RAILWAY SYSTEM—No. II.

TO THE EDITOR OF THE MINING JOURNAL.

SIR,—I am in possession of one of the late Mr. James's pamphlets, published in 1823, in which he recommends conveying passengers, and speaks of sending the crew, ordnance, anchors, and stores from Chatham to Portsmouth, &c. This certainly proves that it was railways, not tramroads, that Mr. James advocated, and he was decidedly the first person who entertained the idea of carrying persons by steam locomotion. Mr. James's plan, before mentioned, is the one he surveyed in 1820, at his own expense, which he intended to extend from Staffordshire to London; and again, his plan of railway from Portsmouth, Chatham, Rochester, Shoreham, Brighton, and London: these would have been rather lengthy tramroads to have been drawn by horses, or even by Mr. Stephenson's "iron horse," to travel at the rate of "six miles an hour," and, probably, we might have been creeping at that pace still if Mr. W. H. James, C.E., had not provided that same horse with "iron intestines." The poor hollow boiler might have puffed and puffed in vain to get more speed. Mr. Smiles says "George Stephenson's claim to be called the inventor of the locomotive in the ordinary sense of the word is clear and indisputable, but in a broader sense it is true, as his son Robert said at Newcastle, that the locomotive is not the invention of one man." Possibly Mr. Smiles knew better than Mr. Robert Stephenson; and in the "Lives of the Engineers" Mr. Smiles states "that the Manchester and Liverpool line of railway was Mr. Sander's project." So, for what can all the volumes and volumes have been published, and monuments erected to Mr. Stephenson? Mr. Smiles certainly endeavours to impress the public mind that Stephenson was the principal projector and inventor. "The Life of George Stephenson" was written for that purpose; "Self Help" was for the same purpose; and now the "Lives of the Engineers." We consider Mr. Stephenson's greatest merit is in having been a successful man. Mr. Hudson was made a Railway King through being a successful man. They were not "bold, reckless" men, like James; they knew what they were about. Some persons are born to believe; they care not for impossibilities or contradictions, so that it is an amusing work, and written by a person with a name. They will believe any absurdity from a favourite author, and often pass by works of merit. How can we reconcile ourselves that Mr. Pease, a quaker, after having had an interview with Mr. Stephenson, should have directed his letter—"G. Stephenson, Esq., Engineer." And how could Mr. Pease have been so unthinking as to expect an answer, for we must recollect that at this period Mr. Stephenson was repairing his neighbours' watches and clocks, mending colliers' coats, boots, and shoes, and working the engine to enable him to defray the expense of his son's education at the University; and Mr. Pease was unfeeling enough to expect this poor overworked man to send a reply—and rather a difficult epistle to answer, even some of our engineers of the present day must think. Mr. Pease expected Mr. George Stephenson's answer, "stating the practicability of the line laid out by Mr. Overton, and to state his suggestions as to any deviation or improvements in its course, together with estimates and comparative expenses." "In short," said Mr. Pease, "we wish thee to proceed in all thy levels, estimates, and calculations," &c.—"Lives of the Engineers," page 183. "The times, however, went against him," speaking of W. James; "it was thought he was too bold, some considered him even reckless." Now, the question arises—If we had not had this "bold and reckless" James, it is possible we should never have enjoyed the benefit of railways. We seldom have prudent inventors, and particularly had we depended upon the prudent Stephenson, who objected to send the locomotive upon the Merstham tramroad. It observes in the "Lives of the Engineers"—"But anxious though Stephenson was respecting its extended employment, he was too cautious to risk an expedient which might only bring discredit upon the engine." The same volume mentions an agreement entered into by Messrs. Stephenson and Losh, to give James a certain share in the engine, for his recommendation. But to whom could he recommend it, when at that period of time we had no railways? James would not have benefited much even had he supplied every tramroad in England.

We can easily perceive the impression Mr. Smiles endeavours to give his readers when he says "it turned out that the first survey of the Manchester and Liverpool line was very imperfect, and it was determined to have a second one made the following year. Robert Stephenson was sent over by his father to assist in the survey. He was present with Mr. James on the occasion on which he tried to lay out the line across Chat Moss." Mr. Robert Stephenson's assistance many will think occasioned a feeling of confidence, having such an assistant, but when we inform them that Mr. R. Stephenson was at that time not more than 19 or 20 years of age, and it was the first survey he had ever witnessed, we do not think Mr. James could have received much actual assistance from him, the benefit was to Mr. Robert; many still living, and who were on that survey, must know. It is very evident Mr. Smiles wished the public to think differently. In p. 188 of the "Lives of the Engineers" we find a letter from Mr. R. Stephenson to Mr. James, at the time of Mr. James's unfortunate embarrassments, quoted from "The Two James's and the Two Stephensons," dated "*August* 29, 1823.

"DEAR SIR,—It gives rise to feelings of true regret when I reflect upon your situation, but yet a consolation springs up when I consider your persevering spirit will for ever bear you up in the arms of triumph, instances of which I have witnessed of too forcible a character to be easily effaced from my memory. It is these thoughts, and these alone, that could banish from my soul feelings of despair for one; the respect I have for him can be easier conceived than described, &c.—ROBERT STEPHENSON."

We will now see the difference to the one Mr. Smiles states Mr. R. Stephenson wrote to him, accompanied with a parcel of letters from Mr. James to Mr. Sanders—no doubt a private communication between these two gentlemen. We cannot understand how Mr. R. Stephenson became possessed of them. "There is a bundle of James's, which characterises the man very clearly as a ready, dashing writer, but no thinker at all on the practical part of the subject he had taken up. It was the same with everything he touched; he never succeeded in anything, and yet possessed a great deal of taking talent. His fluency of conversation I never heard equalled, and so you would judge by his letters." If this was indeed written by the late R. Stephenson, we can only say "what a falling off was there."

With these records we are certainly combating with the weapons of the dead, and we feel that this subject is far too delicate and sacred to handle without every regard for truth. We admit that our interest is created for the memory of a respected and beloved friend, but may we not enquire—For what does Mr. Smiles write? C. E.

THE TWELFTH ANNUAL STATISTICS OF THE MINING INTEREST.

BY WILLIAM HENRY CUELL, ESQ.

TABULAR STATEMENTS, CONTAINING RETURNS OF ORE, &c., FROM DIVIDEND-PAYING MINES, FOR THE PAST YEAR (1862).

CORNISH AND DEVON MINES.

Shares	Name of Mine.	Amount Paid.	Market value.	Dividend per share.	Total Amount.	Metal.	Parish.	Purser or Secretary.	Address.	System.	Meetings, when held.	Copper.	Tin.	Lead.	Total Amount of Money.	Lease granted.	Dues.
		£ s. d.	£ s. d.	£ s. d.	£							Tons.	Tons.	Tons.	£ s. d.	In Years.	
4000	Bedford United	2 6 8	4 0 0	0 8 6	1700	Copper	Tavistock	T. B. Laws	50, Threadneedle-street	Cost-book	Three months	2558	—	—	12,269 10 1	1853 .. 21	1-12
240	Boscean	20 10 0	47 0 0	1 0 0	240	Tin	St. Just	S. York	Penzance	ditto	ditto	—	152	—	10,526 0 0		1-25
200	Botallack	91 5 0	270 0 0	12 10 0	2500	Tin and copper	St. Just	J. H. James	St. Just	ditto	Two months	520	362	—	28,745 0 0	1859 .. 21	1-24 18
916	Cargoll	14 15 7	40 0 0	1 0 0	916	Silver-lead	Newlyn	E. Michell	Truro	ditto	Three months	—	—	1130	16,064 10 3		1-16
1000	Carn Brea	15 0 0	50 0 0	2 0 0	2000	Copper and tin	Illogan	F. Rochfort	Queen-street-place	ditto	Irregular	2243	522	—	40,642 0 0		1-19
2900	Clifford Amalgamated		21 0 0	2 16 0	7719½	Copper	Gwennap	Williams and Son	Truro	ditto	Two months	14322	—	—	68,485 3 0		
2450	Cook's Kitchen	15 18 9	32 0 0	0 14 0	1716	Tin	Illogan	R. H. Pike and Son	Camborne	ditto	Four months	—	303	—	20,542 4 9	1860 .. 21	1-24
256	Copper Hill	51 0 0	70 0 0	7 0 0	1792	Copper	Redruth	S. and R. Davey	Redruth	ditto	ditto	1425	9	—	6,419 9 4		1-15
1055	Craddock Moor	8 0 0	28 0 0	1 0 0	1055	Copper	St. Cleer	J. Taylor	Liskeard	ditto	Two months	1904	—	—	12,524 6 3	1861 .. 21	1-15
1024	Devon Great Consols	1 0 0	500 0 0	52 0 0	53248	Copper	Tavistock	A. Allen	Gresham House, Old Broad-st.	Joint-stock	ditto	24615	—	—	116,946 4 6	1859 .. 27	1-12
358	Dolcoath	128 17 6	550 0 0	45 0 0	16110	Copper and tin	Camborne	C. Thomas	Camborne	Cost-book	Two months	4521½	994	—	68,065 13 3*	1849 .. 21	1-24
12800	Drake Walls	2 0 0	1 0 0	0 1 6	960	Copper and tin	Calstock	H. Williams	Winchester-buildings	ditto	Three months	1	250	—	17,450 18 8	1861 .. 21	1-30
512	East Basset	29 10 0	55 0 0	11 0 0	5632	Copper	Redruth	W. Richards	Redruth	ditto	Three months	1547	—	—	10,511 5 7†		1-15
6144	East Caradon	2 14 6	42 0 0	3 7 6	20736	Copper	Linkinhorne	C. R. Norton	Salisbury	ditto	Three months	5256	—	—	36,602 12 3	1852 .. 21	1-20*
128	East Pool	24 5 0	400 0 0	15 0 0	1920	Copper and tin	Camborne	W. Frisk	Camborne	ditto	ditto	2950	107	—	17,396 0 0‡		1-15
5000	Frank Mills	3 18 6	4 5 0	0 2 0	500	Lead	Christow	C. Wescomb	Exeter	ditto	Two months	—	—	628	9,130 4 2		
1798	Great Wheal Fortune	31 2 7	31 0 0	2 0 0	3596	Tin	Breage	T. W. Robinson	Hayle	ditto	Three months	10	448	—	31,921 19 11	1852 .. 21	1-20 18
5908	Great Wheal Vor	40 0 0	50 0 0	0 10 0	2954	Tin	Helston	G. Noakes, man. direc.	Gresham House, Old Broad-st.	ditto	ditto	—	223	—	15,466 0 0		
10768	Gunnis Lake	0 2 0			768	Copper	Calstock	R. T. Skinner	Tavistock	ditto		607	—	—	3,272 0 0		
1024	Herodsfoot	8 10 0	50 0 0	5 5 0	5376	Silver-lead	Lanreath	T. Trevillion	Lanreath	ditto	Four months	—	—	682	14,540 3 6		
9000	Marke Valley	4 10 6	9 0 0	0 18 0	8100	Copper	Linkinhorne	J. Harding	Salisbury	Deed of Set	Three months	4811	—	—	19,391 4 7	1851 .. 21	1-18†
6000	North Downs	2 5 0	2 12 6	0 2 6	750	Copper	Redruth	Dunsford & Ranken	Broad-street-buildings	Cost-book	Four months	1338	—	—	8,912 16 8		1-20
5936	North Treskerby	1 6 3	4 0 0	0 3 0	869½	Copper	Redruth	B. Matthews	St. Day	ditto	Three months	2323	—	—	11,831 19 0		
6400	Par Consols	1 2 6	5 0 0	0 7 0	2240	Copper and tin	St. Blazey	Major Davis, R.M.	St. Blazey	ditto	Four months	3133	297	—	44,887 13 7		1-16
1120	Providence	10 6 7	42 0 0	4 10 0	5040	Tin	Lelant	S. Higgs	Penzance	ditto	Three months	10	335	—	21,906 9 11		1-22
6000	Rosewall Hill and Ransom United	2 16 0	3 10 0	0 8 6	2250	Tin	Towednack	T. Treweeke	Uny Lelant	ditto	ditto						1-16
1024	Rosewarne Consols	3 7 6	3 10 0	0 2 0	402½	Copper	Gwinear	J. Hollow	Lelant, Hayle	ditto		374	—	—	3,358 0 1		
940	St. Ives Consols	8 0 0	25 0 0			Tin	St. Ives	T. Treweeke	Uny Lelant	ditto	Three months						
512	South Caradon	1 5 0	400 0 0	30 0 0	15360	Copper	St. Cleer	T. Kittow	Liskeard	ditto	Two months	5459	—	—	51,112 5 1	1862 .. 21	1-18
6000	South Exmouth	1 0 0	5 0 0	0 5 0	1500	Lead	Christow	C. Wescomb	Exeter	ditto		—	—	486	6,153 11 11		
496	South Frances	18 18 9	92 10 0	7 0 0	3472	Copper and tin	Illogan	J. Cady	Camborne	ditto	Two months	3442	18	—	16,369 17 7		1-15
512	South Tolgus	8 0 0	40 0 0	3 10 0	1792	Copper	Redruth	J. Haye	Redruth	ditto	ditto	2508	—	—	16,274 9 8		
6000	Tincroft	9 0 0	18 0 0	1 0 0	6000	Tin and copper	Pool, Illogan	H. Williams	Winchester-buildings	ditto	Three months	1649	304	—	24,974 6 7	1862 .. 21	1-24
1000	Trumpet Consols	11 10 0		2 0 0	2000	Tin	Wendron	T. Julian	Helston	ditto						21	1-20 24
6000	West Basset	1 10 0	12 0 0	1 6 0	7800	Copper	Illogan	W. A. Buckley	50, Threadneedle-street	ditto	Two months	5439	10	—	31,743 0 6	1852 .. 21	1-15
1024	West Caradon	5 0 0	27 0 0	1 10 0	1536	Copper	St. Cleer	Dunsford & Ranken	Broad-street-buildings	ditto	ditto	2636	—	—	18,715 18 10	1854 .. 21	1-15
6400	West Fowey	7 18 0	4 0 0	0 5 0	1600	Copper and tin	St. Blazey	Major Davis, R.M.	St. Blazey	ditto	Four months	225	192	—	14,174 17 3		1-16
400	West Wheal Seton	47 0 0	290 0 0	38 0 0	15200	Copper	Camborne	B. Matthews	St. Day	ditto	Two months	6284	—	—	37,698 17 8§	21	1-15
512	Wheal Basset	5 2 6	80 0 0	15 0 0	7680	Copper and tin	Illogan	W. Richards	Redruth	ditto	ditto	2715	47	—	20,778 11 2	1849 .. 21	1-15
1024	Wheal Gryils	1 10 0	30 0 0	1 10 0	1536	Tin	Perranuthnoe	Dunsford & Ranken	Broad-street-buildings	ditto	Three months	—	—	—	11,385 5 1		1-18
1024	Wheal Hearle	9 13 8		0 5 0	256	Tin	St. Just	J. Hollow	Lelant, Hayle	ditto		38	82	—	5,544 4 9		
512	Wheal Jane	3 10 0	15 0 0	2 0 0	1024	Tin, lead, and mundic	Kea	J. Tippett	Truro	ditto		£303¾	£5080	£655	7,654 8 3‖	1852 .. 21	1-18
4800	Wh. Ludcott & Wrey Con.	2 10 8	9 0 0	0 10 0	2400	Silver-lead	St. Ive	J. Taylor	Liskeard	ditto	Three months	—	—	580	22,950 10 9¶	1852 .. 21	1-12
896	Wheal Margaret	9 17 6	40 0 0	5 5 0	4704	Tin	Uny Lelant	T. Treweeke	Uny Lelant	ditto	ditto	—	258	—	15,841 11 8		
1024	Wheal Mary Ann	8 0 0	16 0 0	2 0 0	2048	Silver-lead	Menheniot	P. Clymo	Liskeard	ditto	Three months	—	—	910	19,813 0 0		1-15
1024	Wheal Kitty	1 7 2	7 0 0	0 10 0	512	Tin	Uny Lelant	T. Richards	Camborne	ditto		—	105	—	6,891 19 6	1852 .. 21	1-15
80	Wheal Owles	70 0 0	300 0 0	25 0 0	2000	Tin	St. Just	J. Boyns	St. Just	ditto	Three months	—	273	—	18,831 13 4		1-25
396	Wheal Seton	107 0 0	100 0 0	10 10 0	4554	Copper	Camborne	T. H. Tilly	Falmouth	ditto	Two months	3790	—	—	17,271 19 6		1-15
1040	Wheal Trelawny	5 10 0	17 0 0	2 7 6	2470	Silver-lead	Menheniot	Dunsford & Ranken	Broad-street-buildings	ditto	Three months	—	—	1012	23,128 1 1		1-15 20

* This includes arsenic sold for 129*l.* 10s.
† This amount includes tinstuff, which realised 1226*l.* 9s. 10d.
‡ This amount includes 104 tons of arsenic, which realised 104*l.*
§ This amount includes tin sold, which realised 1495*l.* 14s. 6d., and arsenic 14*l.*
‖ This includes 875 tons of mundic, which realised 714*l.* 12s. 9d.
¶ This includes 191 tons 18 cwts. 2 qr. of silver ores, which realised 14,584*l.* 14s. 8d.
* A new lease has been obtained for 25 years.
† Arrangements have been made for a renewal, at same dues.

WELSH MINES.

Shares	Name of Mine.	Amount Paid.	Market value.	Dividend per share.	Total Amount.	Metal.	Parish.	Purser or Secretary.	Address.	System.	Meetings, when held.	Copper.	Tin.	Lead.	Total Amount of Money.	Lease granted.	Dues.
867	Cwm Erfin	7 10 0	9 0 0	1 10 0	1300¼	Lead	Cardiganshire	J. Taylor and Sons	6, Queen-street-place	Cost-book		—	Blend	609	9,195 15 10		
128	Cwmystwith	60 0 0	105 0 0	16 0 0	2048	Lead	Cardiganshire	J. Taylor and Sons	6, Queen-street-place	ditto		—	—	1210	14,958 19 4		
3000	Dyfngwm	12 6 6	10 0 0	0 10 0	1500	Lead	Penegais (Mont.)	G. Hadley	8, Old Jewry	ditto		—	—				
300	East Darren	32 0 0	40 0 0	8 0 0	2400	Lead	Cardiganshire	J. Taylor and Sons	6, Queen-street-place	ditto		—	—	855	12,913 10 0		
400	Lisburne	18 15 0	100 0 0	23 0 0	9200	Lead	Cardiganshire	J. Taylor and Sons	6, Queen-street-place	ditto		—	—	3493	44,472 12 4		
1800	Minera	25 0 0	250 0 0	21 15 0	39150	Lead	Wrexham	J. Frazer	Wrexham	ditto		—	805	6410	82,860 5 0		

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